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The Role of Rothamsted in Making Nutrition a Science

THOSE who were concerned with victualling military expeditions, voyages and migrations, were in the past hazy about food requirements. Catastrophe was often prevented only by pillage or, according to legend, by the opportune appearance of quails or manna, or by ill-advisedly eating Hyperion's cattle. Total or partial starvation was common (e.g. Watt *et al.*, 1981). This haziness is curious because many institutions kept records of the food supplied to the inmates; some of these must have been available in antiquity, as records are for more recent times, e.g. those cited by Hallam (1988) and Hoch (1982). Economical victuallers probably thought that much of the recorded food was wasted or diverted. They also thought, as we do today, that daily allowances such as 0.5 kg meat and 2 kg bread (about 20 MJ) were excessive and therefore disregarded or disbelieved the records. John Milton, in 'Areopagitica', wrote of 'glutton friars'; that opinion was probably widespread, so that monastic corrodies were disregarded. Another difficulty was the absence, until the time of A. Lavoisier, of the idea that food is a fuel and that all foods could therefore, in principle, be grouped together.

An important step towards unification was taken by John Wilkins (Figure 1), the Secretary of the infant Royal Society (1663–1668). He wanted to know how many animals he had to design symbols for in his proposed universal language. So, being a bishop who believed the Bible, he thought that the dimensions of Noah's ark gave a maximum number. By the time of the flood, some animals were thought to have become carnivorous. So he devised 'sheep units'. The larger herbivores were assigned several units each, the carnivores a suitable number, and the necessary amount of hay for all the actual or notional sheep was calculated. He found that there would be room in an ark of the specified dimensions, during a voyage of the specified duration, and was thus able to refute 'some atheistical scoffers'. The relevance in the present context is that he thought quantitatively of a food unit. When, about 1809, A.D. Thaer introduced his 'hay equivalents', he seems not to have known that he had a forerunner.

It is surprising that B. Thompson (Count Rumford) wrote such nonsense about diet because, although he wrote before the enunciation of the 'Conservation of Energy', he had clearer ideas

about energy than most of his contemporaries. Queer ideas about the nutritional potentialities of properly cooked water, made him suggest that a bowl of soup, supplying little more than 1 MJ daily, would maintain a man working on one of his treadmills. Physiologists, notably C. Voit, disagreed and argued that people needed what they usually ate; they therefore increased Rumford's estimate 10 or even 20 fold. But some scientists clung to ideas similar to Rumford's. Some people still do. During the 1914–18 war G. Lusk (1928), who had studied with Voit, and R. H. Chittenden, lived for some weeks in Britain on rations approximating to Chittenden's abstemious ideal. They then got some meals on a US naval transport. Lusk attributed their feeling of well-being to the improved diet. Chittenden to the sea air. Even at that date, in spite of the work of J. B. Lawes and J. H. Gilbert, nutritional opinion depended largely on the mixture of assumption and ideological prejudice, which dependence had earlier been nearly complete.

Lawes wrote two autobiographical notes (1873; 1888). Like many productive and original scientists, he took little



J.B. Lawes

Figure 2: Engraving of Lawes, circa 1842 by an unknown artist. Print courtesy of Rothamsted Photographic Department.

interest in school work; he left Oxford without a degree. If formal education had interested him more, he might have taken more care over the presentation of joint papers. Many sentences are hard to disentangle—one (perhaps significantly on sewage sludge) rambles for 20 lines plus an eight line footnote. In adolescence he was an enthusiastic chemist and later extracted active principles from belladonna, colchicum, hemlock, henbane, poppy etc., which he grew on the Rothamsted Farm, where he was born and which he subsequently inherited. Use of these products is not recorded. He described himself as 'always in mischief', 'passionately fond' of shooting, and '... took no part in the sports of the school such as football, cricket...'. He carried on these experiments in spite of the passionate objections of his widowed mother. In Paris in 1830, during the revolution which ousted Charles X and established Louis Philippe, he '... thought it great fun and helped to build the barricades'. His interest in agriculture and in the response of plants to different types of manure was aroused by a neighbour telling him that bones improved growth on some fields but not on others.

Rothamsted Farm made little money: Lawes' wealth came from the manufacture of superphosphate from bones and coprolite, and from patents on the process. Fortunately for the future of agricultural research he defended these patents successfully although there was

little originality in them, and was thus able to finance his research and set up the Lawes Agricultural Trust.

Controversy

As the patent dispute shows, Lawes was eager to stand up for his rights and get the royalties due to him. On scientific matters he was a willing but not eager controversialist. Though often in conflict with J. Liebig, he differed from others by being content with a bald statement without polemics. Others, e.g. R. DuBois-Reymond and J. J. Berzelius, were less restrained. Even the usually mild F. G. Hopkins wrote (1936) 'Liebig himself... though so brilliant a chemist, lacked biological training and, as I have always felt, a biologist's instincts.'

Agriculture supersedes Nature

Early in his research career, Lawes stated his objective clearly (1847): 'Agriculture will eventually derive the most important assistance from chemistry, but before it can propose any changes in the established routine of the farmer, it must, by a series of laborious and costly experiments, explain this routine in a satisfactory manner.' Earlier in that paper he had pointed out that argument from first principles and from what happened naturally was fallacious because 'Practical agriculture consists in the artificial accumulation of certain

constituents to be employed either as food for man or other animals, upon a space of ground incapable of supporting them in its natural state.' He did not go on to say, as we would do now, that in the process we should avoid doing too much harm.

Work on nutrition started with comparisons of the performance on different diets of different breeds of sheep, it was later extended to cattle and pigs. With characteristic thoroughness, Lawes & Gilbert (1859) made abundant, perhaps superabundant, measurements. The enormous Tables in their papers, giving weights, percentage dry matter, nitrogen, fat and minerals in most of the organs and commercially differentiated parts of the animals are clear although, at first sight, intimidating. Their approach was reasonable: one cannot see which are the most relevant and significant measurements until everything has been measured.

Before the development of micro-analytical methods, mineral analyses were done on ash from substantial quantities of tissue. During the centenary celebrations at Rothamsted in 1943, Sir Charles Harington asked me if he could see the platinum basins in which Gilbert had incinerated pigs for these mineral analyses. A member of the Biochemical Club, which had visited Rothamsted in 1911, had told him they were as big as washing-up bowls. I asked Sir John Russell, the Director of Rothamsted, what had become of them. He just smiled: he hated seeing wealth lying unused.

Figure 1: Engraving of Wilkins' ark, published in 1668 in his book: 'An Essay Towards a Real Character and a Philosophical Language'. John Wilkins, who was Oliver Cromwell's brother in law, and Bishop of Chester, included in his book a refutation of those who asserted that the Ark as described in the book of Genesis could not possibly have existed. Print courtesy of the Royal Society Library.

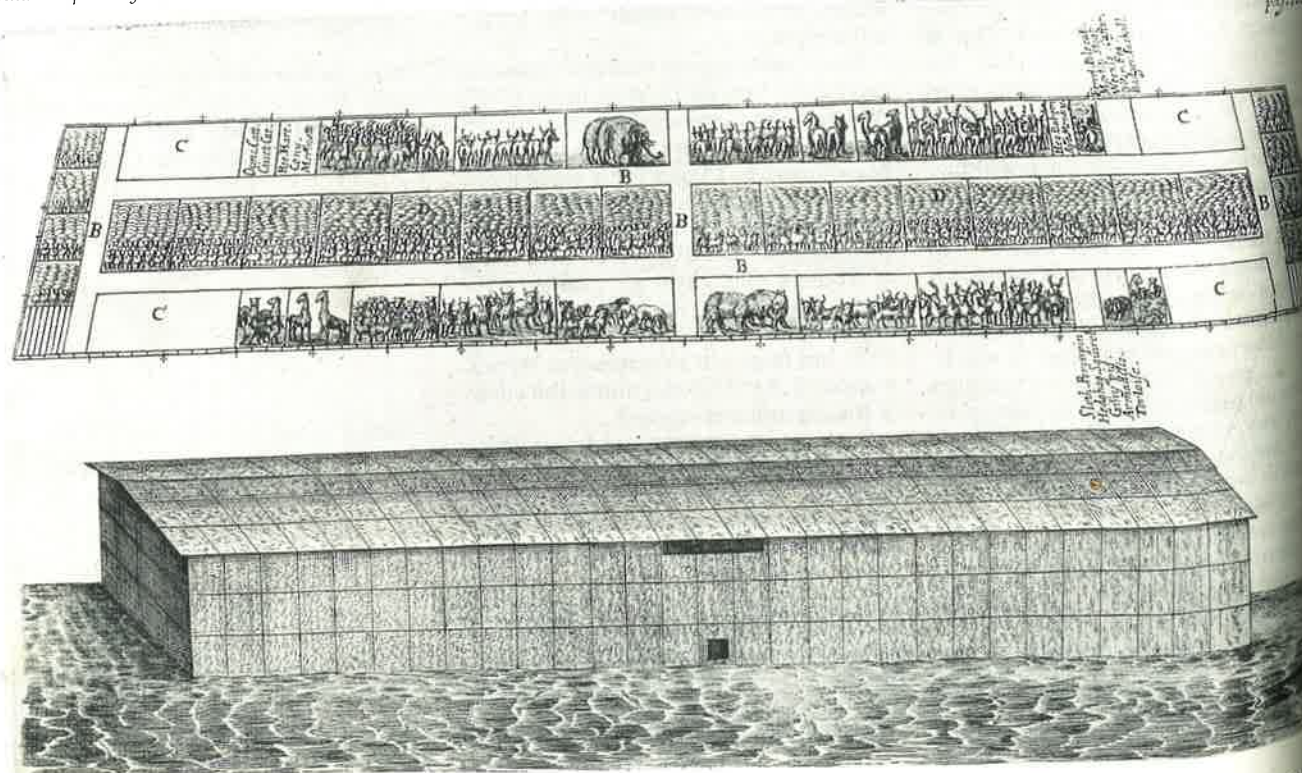


Figure 3: An early barn conversion—Lawes' laboratory, in which Gilbert worked, was set up in 1837, just three years after Boussingault's laboratory at Pechelbronn, Alsace, and was the first of its kind in Great Britain. Photograph courtesy of Rothamsted Photographic Department.

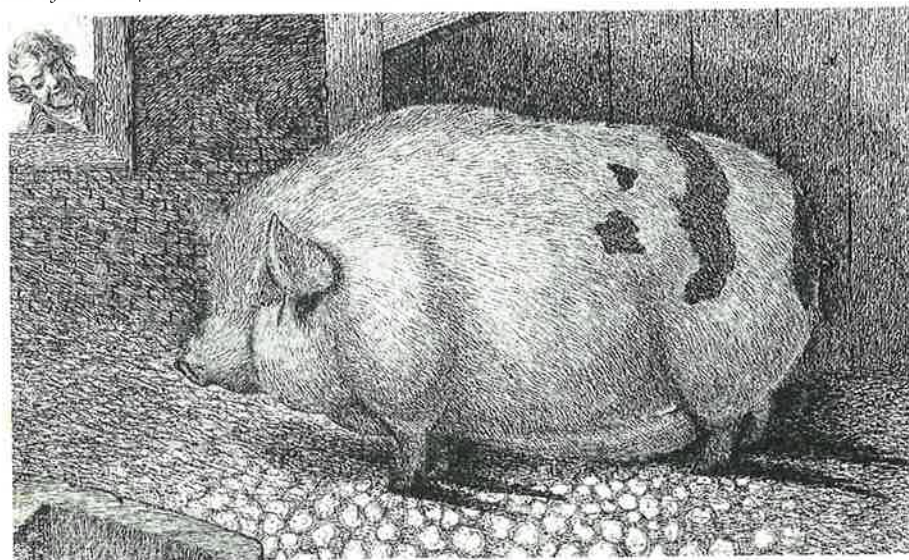


Significance of Protein

G. J. Mulder and Berzelius introduced some terminological clarity into nutrition in 1838 by grouping many of the nitrogen-containing components of wheat, milk, beans, cereals and leaves into the category *protein*. Protein then became a fetish. J. B. J. D. Boussingault produced a table purporting to equate the nutritional value of several foods with their protein content, and it seemed obvious to Liebig that, because the dry matter of muscles is mainly protein, that is their fuel. Most scientists at that time agreed with these conclusions. Scientists are not wholly immune to the popular habit of attributing many activities and properties to exciting novelties.

Lawes and Gilbert disputed the unique importance of protein in human and animal nutrition: they realized that it is essential to get adequate amounts of all the then-known dietary components. As they put it (1853) '... it cannot be doubted, that beyond a limit below which few, if any, of our current fattening foodstuffs are found to go, it is their available non-nitrogenous constituents rather than their richness in nitrogenous ones, that measure both the amount consumed to a given weight of animal, within a given time, and the increase of weight obtained'. They continued: 'A somewhat concentrated supply of nitrogen does, however, in some cases, seem to be required when the system is overtaxed; as for instance, when day by day, more labour is demanded of the animal body than it is competent without deterioration to keep up; and perhaps also, in the human body, when under excitement or excessive mental exercise.'

Figure 4: The face at the window is not Lawes. This imaginative drawing of a pig was done about 1800: it may have inspired Lawes. Print courtesy of Rothamsted Photographic Department.



Eulogy on Pigs

The word *few* in the first quotation is important. Because of expense, they did not take sugar seriously as a feed and knew nothing of cassava and similar fodders which contain very little protein. They thought of cereals as the main feed. When reading their papers, this has constantly to be borne in mind: by a non-nitrogenous feed they usually mean one which contains 8 to 12% protein. As Lawes put it (1864) '... if I were asked to state, in general terms, what was the approximate proportion of the nitrogenous to the digestible non-nitrogenous substances, below which they should not exist in the food of our stock, I should say (though with reservations) about such as we find them in the cereal grains'. With a rare touch of humour he later (1885) wrote of '... barley meal—which I might call the natural diet of civilized pigs'. His respect for pigs is often shown. He calls the pig '... his useful friend' and attributes to pigs, even more than to sheep and cattle '... an unerring instinct which enables them not only to distinguish between substances which are and are not food, but also to select from a variety of food stuffs those which are most suitable for the requirements of the system'. Lawes and Gilbert observed that the intake of protein concentrates by animals, given some choice in their diets, varied much more than the intake of such foods as cereals. They trusted the animals' 'unerring instincts' more than the nearly unanimous assumptions of other scientists. They trusted 'the market' almost as much as they trusted the pig and wrote (1853) 'Thus the comparative prices of the leguminous seeds and the cereal grains, may be taken as a pretty

safe condemnation of the measurement of feeding value according to their percentage of nitrogenous constituents.'

Conversion of Carbohydrate into Fat

An important conclusion from their early analyses was that meat was an overrated protein source. That was mainly a result of their meticulous attention to percentage dry matter. Unfortunately, some modern writers do not have this habit. A few absurd claims depend on comparisons between values for one crop while fresh and another after drying. Because fat contains little water compared to muscle, a piece of meat which seems to be only one third fat, will contain about equal amounts of fat and protein.

They concluded (1859) that in meat 'respiratory or fat-forming material' is 1.5 times that in bread. They did not think that this detracted in any way from the value of meat in the diet. They accepted that this did not depend solely on the protein in it. As they put (1858a) 'It remains to Physiology to lend her aid, to the full explanation of which Chemistry and common usage have thus determined'. They repeatedly, almost obsessively, stressed that most people chose fatty cuts of meat and they assumed that the fat was needed. They were mainly concerned with the diets of hard-working people living in underheated houses; they also pointed out that the wealthy, who tended to choose lean meat, 'consume the most butter, sugar, and in many cases, alcoholic drinks also' (1853).

It was widely assumed, for obscure reasons, that animals are purely catabolic and lack the capacity for synthesis. That absurd assumption should have disappeared in 1841 when A. Ure found that animals synthesized hippuric acid when given benzoic acid. Traces of it lingered on to the start of this century and generated work on substances such as mercapturic acid. Those who believed in the assumption were puzzled by the presence of marginally more fat in some animals than had been present in the food. Lawes and Gilbert's analysis showed that pigs could contain many times as much fat as they had eaten. This made the problem acute. Others, working with dogs, followed the then usual policy and suggested protein as the source. Lawes and Gilbert did not do that, but demonstrated the impossibility of all the fat being made that way and argued that most of it came from carbohydrate (1853). Liebig, for theoretical reasons, had suggested otherwise still disagreed. Lawes and Gilbert

therefore wrote two papers (1866b; 1877) dealing mainly with the formation of fat. They surveyed the state of the controversy, stressed that the voracity of pigs and their readiness to fatten made them peculiarly suited for this research, and reassembled their earlier measurements. Although they argued that further experiments were not needed, they explained how they ought to be done. That settled the matter: carbohydrate was accepted as the main source of fat—at any rate. Lawes stated confidently (1887), in a note called 'The pig of the future', that skilled animal breeders who had already made from a long eared, long legged, hairy Greyhound species of animal—a pig resembling a ball of hairless fat... would be able to produce whatever type of pork was required. In that late paper Lawes argued that people still wanted as much fat as before, but no longer liked getting so much of it in meat.

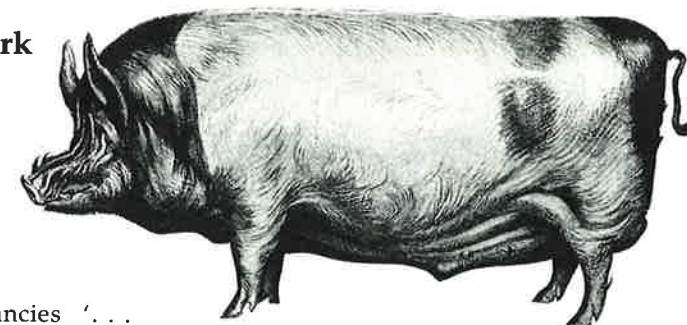
Fuel for Muscular Work

That episode of agreement with Liebig on the source of fat was unusual. Nevertheless, the phrase often repeated in essence (e.g. 1854), that theoreticians have attributed discrepancies '... rather to erroneous interpretations of common practice or experiment, than to any defect in the theoretical method of estimation', was not aimed specifically at him. Lawes and Gilbert seem to have thought it unnecessary to contradict directly Liebig's assumption that protein was the source of muscular energy. Their already quoted remarks show clearly that they did not doubt the importance of protein—they doubted its pre-eminence. They were aware (1853) that protein had been suggested as the means for the 'exercise of force', and they had dismissed the suggestion that differences in the activity of test animals could explain differences in dietary response. Their primary concern was to refute such statements by Liebig as 'The sum of the mechanical effects produced in two individuals, in the same temperature, is proportional to the amount of nitrogen in their urine'. Until A. Fick and F. J. Wislicenus's experiment got considerable publicity in 1866, the nearest they came to describing an experiment on the effects of muscular activity on metabolism was a cryptic footnote (1859) saying that '... with animals kept almost entirely without movement; ... most of the nitrogen was

excreted as urea.'

A Colourful Experiment

Fick and Wislicenus, while climbing 2000m on the Faulhorn, collected their urine. It contained no more nitrogen than urine collected in a similar period at rest. This was the most colourful of several similar experiments all tending to refute Liebig. Lawes and Gilbert clearly felt that they had already done that by their general attitude to dietary protein. Nevertheless, they published (1866b) an experiment done in 1854 in which equally inactive pigs were fed either on lentils containing 4% nitrogen or barley containing 2%. They found that '... the amount of nitrogen voided by fattening animals fed under equal conditions as to exercise of force, bore a very direct relation to that supplied in the food'. This was done several times



and urea was found to be the main form in which nitrogen was excreted. This experiment is sometimes wrongly reported. It was concerned with the quantity not the quality of food protein; different sources of protein had to be used simply to get the necessary large nitrogen intake. By hindsight, the result seems pretty obvious, but at that date (c.f. Gilbert 1895) there was no general agreement that atmospheric N_2 played no part in mammalian metabolism, and that food nitrogen was not exhaled as N_2 . Also by hindsight, it is a pity a third diet, of lentils diluted with starch, was not tried—or if tried was not published.

Obviously, the problem can be approached in two ways. Fick and Wislicenus assumed that their nitrogen intake was constant and showed that nitrogen excretion did not increase when the work done was increased. Lawes and Gilbert assumed that the amount of work done was constant and showed that nitrogen excretion depended on the amount in the diet. Their experiments were always designed in ways that made agricultural sense. Although they often used child labour, they would not have

thought it made sense to employ children to chivvy a group of pigs round a pen and thus clinch the matter. For some experiments they had a metabolic cage with a bell on it which recorded movement (Gilbert 1895; Grey) that was used only when records of a pig's defecation were being kept.

Experiments and Statistics on Wheat

Controversy arising from Lawes and Gilbert's papers on fat synthesis, and on the fuel for muscular work, made their opinions well known. That, and the impressive accumulation of supporting measurements, arguably the largest ever presented on one theme, did much to make nutrition a scientific subject rather than a collection of anecdotes. Their work on the manurial requirements of wheat is very well known; they were concerned with wheat in other ways. Every year from 1863 to 1899 Lawes sent a letter to *The Times* commenting on the wheat crop. Grey (undated) describes the pressure he put on the laboratory staff at harvest time to get the Rothamsted yield figures out quickly. After Sir William Crook's celebrated forecast to the British Association of impending starvation, Lawes and Gilbert wrote (1898) a spirited rebuttal; it included the forecast '... there is no doubt that there still exists in the United States great inherent capability of production of wheat, not only for home consumption, but for export also, for many years to come.' They were as devoted to wheat as to pigs and argued (1859) that bread and meat were the basis of a good diet because the mixture was nutritionally better than either food alone.

For many years, grain from differently manured strips of Broadbalk, on which wheat is grown continuously, was analyzed and 50 to 100 kg samples were milled in commercial conditions. Differences in each year's weather, and in the manurial treatments, obviously affected yield; they had little effect on chemical composition (1858b). Although publications on human diets were promised (1853), they did little work on that subject. Their paper on the composition of wheat (1858b) comments on the increased peristalsis caused by wholemeal bread and suggests that it is better suited to the sedentary than the working population. Those who remember the heated controversy on this subject in the 1940s will have a sense of déjà vu on reading that paper.



Figure 5: Rothamsted Experimental Station as it is today. Photograph courtesy of Rothamsted Photographic Department.

General Outlook

That feeling is aroused by some other comments by Lawes which deserve mention although they have little connection with nutrition. He was a member of the Royal Sewage Commission which was set up to consider the perennial problem of stinking rivers and fouled beaches. His first advice (1855), based on a survey of published analyses of sewage from many sources 'Use it for irrigation'. He and Gilbert then (1866a) published the results of trials showing that, on land with suitable gradients, the grass yields increased four or five fold. Lawes' casual comment (1864) that use of fossil fuels and destruction of forests were increasing atmospheric CO₂, but that '... more carbon is fixed in an acre of luxuriant wheat than over the same area of woodland'; so that agriculture could redress the balance, is unexpected and prescient. All that was missing to bring this comment up to date is a reference to the 'greenhouse effect'.

At the risk of the accusation of 'nit-picking' it can be argued that Lawes, although he intended to belittle the importance of proteins compared to carbohydrate or fat, tended to have romantic ideas about them. For example; protein in leaves was judged inferior to that in grain because 'less elaborated' (1847), so was the protein in bran (1858b). He wrote favourably (1854) of the '... already animalized protein compounds supplied in cod-fish, ...'. He showed no such tolerance towards manufactured foods and condiments, which were beginning to be widely advertised for use by farm animals (1858; 1862). They cost five to ten times as much as conventional feeds and gave no better growth rates or conversion efficiencies.

He did however concede that they might be useful with old or debilitated animals. He emphasized that the apparent analogy with artificial manures was invalid. With them, the same amount of the essential elements are applied as is applied with dung, but dung is bulkier and wetter. Oilcakes likewise are not analogies. They are a by-product of oil production and are therefore sold at arbitrary prices which depend on the price of equivalent feeds (1858).

Someone seems to have revived the suggestion, made by T. Beddoes in 1792, that human food could be extracted from leaves. Lawes accepted the possibility (1885) but argued that ruminants are adequately equipped to do the extraction themselves. It is hard to understand how anyone could have suggested sugar as a cheap feed in Britain. However, Lawes and Gilbert (1855) demonstrated that it, and malted barley, were no better than starch when allowance was made for the 20% water in starch. Pigs liked sugar—so did farm staff (1885).

Lawes may have been romantic about proteins, but he believed neither in Liebig's rational world in which assumptions could be trusted, nor in magic. He repeatedly stressed that everything, especially water content, had to be measured. He wrote (1862): 'If feeders were more accustomed to the use of scales and weights, and had arrived by experience at definite estimates of the amount of increase they should obtain from given amounts of food, the real value of the so-called condimental foods would have been settled in six months after their introduction.' It is interesting that in a paper addressed to professionals (1858) he felt it necessary to write, and italicize, that growth etc. must come from '... constituents

actually contained in the food.' These comments on the intellectual climate of the time justify my choice of a title for this paper.

I am grateful to G. V. Dyke for letting me see the typescript of his annotated bibliography: 'John Bennet Lawes: the Record of his Genius', published by Research Studies Press, Taunton.

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¹ Having noticed several recent patents on points made in H. M. Rouelle's paper of 1773 on the extraction of protein from leaves, and one in 1945 on the method for making glutathione which I published in 1930, I have a rather jaundiced opinion of patent law.

Figure 6: John Bennet Lawes (1814–1900). Photograph courtesy of the Royal Society Library.



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